I shall try to consider the botanic trinomial, not from the ethical point of view as Mr. Townsend seems to have done, but from the taxonomic strictly.

We find it convenient to give a name to a plant simply because the use of the name serves to call up an aggregate of characteristics when we wish, without the necessity of detailing those characteristics. The whole matter is one of convenience simply, and a name means nothing more than this.

It has been pretty universally agreed that it is more convenient to have a binomial name than a monomial one, for by this means we are enabled easily to group our plants, the first name serving to call to mind the aggregate of characteristics of the group (genus), possessed often by many sub-groups (species), and the second those characteristics possessed to a greater or less extent by the individuals that go to make up the sub-group.

So far this seems to be reasonable enough, and, following the same lines, should we choose to add a third name to our binomial, making it a trinomial, we should naturally do so for the purpose of segregating these sub-groups into still smaller ones (varieties). On this line the addition of terms might rationally be continued to the extent that the facts of observation would warrant.

But we find in the *de facto* botanic trinomial a mixture of two taxonomic principles, instead of the rational following out of the single line indicated by adding to the monomial the second term. Usually the third term is added as a compromise with existing fact, simply to avoid the possibility of having two homonomic binomials, and consists of the name of the person who first published the binomial. It is evident that this addition of such a third term serves a purpose only in comparatively rare cases; in the vast majority, were it not for the fear that some future comer would see fit to use the same binomial to designate another plant, it would be, as a name, useless. But at present the addition of the author's name is essentially a part of the naming of the plant.

It is this third name, and comparatively useless one, that is the cause of much of the trouble of the botanic taxonomists. Many seem to feel that this serving as a compromising tailpiece, the necessity for which it is confessedly the aim of the botanic world to do away with altogether, is an honor. And for this reason there is strife in a large class of cases as to the third name to be added to the binomial. For consider the following specific case. Hooker and Arnott notice a plant, which, in their judgment, is a member of the large group of plants that has been called Malva. They therefore give it the binomial name *Malva malachroides*, and first publish the characteristics which that name is to call up. Afterward Gray considers that the plant cannot belong to the group called Malva, and so gives the same plant the name *Sidalcea malachroides*. More recently Greene finds that the plant can be neither a Malva nor a Sidalcea, and calls it *Hesperalcea malachroides*.

Now suppose we have an individual of this group and wish to give it the most convenient name. For the name of the main group undeniably it matters not which of the three names we choose; if we have had the opportunity of studying the plant carefully our choice will be determined by the observed facts and our own judgment. Personally, in the present case, I chose to call the plant Hesperalcea. For the second name there is no choice, the three authors having given it the same. (Had there been a diversity of names here, the name first given the plant would have been chosen, not because this is "just," or "right," but because by this artificial rule we obtain a permanent factor in the name, without fossilizing individual opinion at all regarding the affinities of the plant.)

We now come to the third name, only added, remember, from the fear that some one has called or will call some different plant Hesperalcea malachroides. Here custom is divided, and many would write *H. malachroides, H. and A.*, and others *H. malachroides, Greene*. It is for us now to determine which of these names is the most convenient.[1] The person to whom we wish to communicate the idea, H. malachroides, upon seeing the trinomial H. malachroides, H. and A., naturally turns to the works of H. and A. to find the summing up of the characters of the plant. But here he is met with an insurmountable difficulty. He can find no trace of it. Let him look for *malachroides*, perchance Mr. Townsend would say. But it is easily possible that H. and A. have described five species by the name of *malachroides*. On the other hand, suppose we write H. malachroides, Greene, the person wishing to know of this plant would turn to the works of Greene and there would find the reference to Malva malachroides, H. and A., which would enable him to find the original description of the plant and thus obtain the idea which we wished to convey.

It seems plain enough then that the third name of this trinomial from the standpoint of convenience should be Greene and not H. and A..

Mr. Townsend disposes of this difficulty in the following words:—

"I would write Metsgeria pubescens schrank, . . . and make no more ado or trouble about it. . . . This signifies always that the authority named described the species originally and originally proposed that name. The founder and date of the genus can be ascertained by referring to any monograph."

It is obvious on a little thought that this paragraph assumes a good deal more than the facts warrant. In the first place there certainly will be no monograph of the species named *pubescens*; and it is very possible that a monograph of the generic name chosen may not exist.

But it is perhaps allowable to look at these two trinomials from a slightly different point of view. Which tells the most truth? H. malachroides, H. and A., implies that H. and A. would now choose, as we have done, the group Hesperalcea for this plant. This we have no right to imply; as a matter of fact they did choose Malva, and this is all we know or should state.

Of course, in all the preceding I have assumed that the purpose of a name is to convey from one person to another the idea of a thing, and on this hypothesis it seems to me that the conclusions arrived at are sound; but I would not wish to be understood as desiring that a name should do no more than this. If it can convey the history of the thing, well and good, as long as by trying to do this it does not entirely defeat its own purpose, as I think I have shown Hesperalcea malachroides, H. and A., would do.

C. MICHENER.

San Francisco, Oct. 7.

[1] I have not considered the writing of H. malachroides (H. and A.) Greene, as the parenthetical term is no more an essential part of the name than the date of publication or twenty other particulars which might occur in a monograph on the plant.

Notes on the Saturniidæ, or Emperor and Atlas Moths.

ALTHOUGH the family *Saturniidæ* comprises the largest and some of the handsomest of all the *Lepidoptera*, it is still very imperfectly known. The larvæ are mostly gregarious, and feed on trees. Many of them form cocoons, which are attached to the branches of the trees upon which they live, while others (at least in South Africa) are said to pupate in the ground. I am not certain whether it has yet been ascertained whether this latter habit has been proved to be peculiar to certain species or genera, or whether the same species may form its pupa in different ways, according to circumstances.

There is doubtless a much greater variety of these insects in tropical countries than we are at present aware of. Many of the most remarkable species are only received singly, and often remain unique in our collections for years. Collectors rarely have an opportunity of rearing them from the larvæ, even if they should meet with a brood, and many species probably feed on lofty trees, quite out of reach, while the perfect insects are nocturnal in their habits. Many of the larger, and especially the domesticated species of *Saturniidæ* from which silk is obtained in India, China, and Japan, vary very much, and this is another obstacle to their successful study. Many of these domesticated breeds, and the various wild or semi-domesticated forms allied to them have been simply named, and not described; or perhaps only the food-plants and localities have been indicated. These useless names find their way into our collections and from thence into our lists and papers, and form a wholly unnecessary element of confusion, which should be eliminated as soon as possible, either by the actual description of the species, or by the rejection of these manuscript names. The mischievous practice of attaching names to insects without describing them has long been abandoned by lepidopterists in every branch of the study except sericiculture.

W. F. KIRBY.

London, England, Sept. 25.

Destroying Mosquitoes by Kerosene.

THE reason for the existence of mosquitoes has often been asked. Some means for their destruction has, perhaps, been even more earnestly sought after. The idea that their numbers can be kept down by propagating dragon-flies does not seem to be any longer entertained; and any experiment bearing on some means for their destruction is of interest. In a late number of *Insect Life*, Mr. L. O. Howard publishes a note upon the use of kerosene against them, the substance of which is as follows: On the surface of a pool of water, containing about 60 square feet, he poured four ounces of kerosene. This formed a very thin oily film on the surface of the water. On the 5th of July the pool was teeming with animal life, but for the next ten days that the pool was under observation no living insects were observed. At the end of this time, a count of the insects on a small portion of the surface, from which was estimated the total number, showed 7,400,— 370 of which were mosquitoes. The observation is of interest as showing the remedy to be an effective one, and, further, that a single application of oil will remain operative for ten days or longer, although two rain storms occurred during the interval. The matter is worthy of further observation and experiment.

JOSEPH F. JAMES.

Washington, D.C., Oct. 10.

Phonetics in Science

FOLLOWING almost in the "wake" of the geological word-makers, who have apparently a dictionary of their own construction, comes another scientific writer who has decided to use the phonetic system of orthography. My attention was called to an article in a chemical journal published in this country, and almost at a glance I should have decided, had I not known the system, that the author had just finished writing a translation from the Spanish, and had his alphabet somewhat confused; for here before me was *sulfate;* but reading further, I should have said, perhaps, that he had just finished a German translation.

All this would have occurred to me if I had been ignorant of the existence of the phonetic system. Now, why did not this author change *phenol-phtalein*, which appears in the article referred to? Perhaps this word does not occur in the phonetic dictionary.

Is it not high time for American scientists to stop "coining" words? To be sure, these words differ from the geological ones in that they come well recommended by some philologists, and then the author in this case has not been guilty of owning an "orthographic mint." Why not continue to use the good old spelling, when it answers every requirement? The only disadvantage (?) in so doing, to my mind, may be in the fact that the words are longer than those in the phonetic system, and, as the advocates of this system claim, are more difficult to spell; so they are to some people, but unless they are foreigners, one is not in the habit of meeting such scientists in every-day life. Scarcely has our American language secured a strong foot-hold than it must be changed for the benefit of a few who would receive the honors as the originators and champions of a new system of orthography. I know of one advocate (not the author, it is needless to say, of the paper in the chemical journal above referred to) who "prides himself not only upon his ability to use the phonetic system, but also upon his beautiful English." Yet this very same man habitually uses, for example, such phrases as "Ain't he funny?" Still this hardly belongs to my criticism of phonetics in science. Why not leave the phonetic system to the philologists; why incorporate it in our scientific work?

When the advocates of this system have succeeded in establishing a strong foot hold for their system, and permanency (for it) stares the old system in the face — and let us hope that time is far distant — then we can almost picture our laboring scientists, with the new system (?) dictionary before them, ever fearful of beginning one word with an F after the new, and the next with a Ph after the system they have so successfully used for generations.

E.

Grand-Gulf Formation.

DR. WM. H. DALL'S contribution to Miocene literature under this head calls for some notice, were it only to thank that eminent palæontologist for correcting my mistake with regard to the Gnathodon of Pascagoula and Mobile. With his unrivalled opportunities of comparison and long experience in these studies, his determination is naturally satisfactory and final. I knew that in mollusks the young and the adult forms often differ considerably; but I knew not the life history of this one.

It is complimentary to me also that he has accepted my outline of the evolution of the Florida Peninsula,[1] although he probably arrived at his conclusions from different and independent sources. And I wish to correct the impression he seems to have of my notions of the genesis of the Grand Gulf. I do not say that the Pascagoula is a deep-sea formation, but speak of it as a "marine aspect" of the more intensely fresh-water Grand Gulf on the Mississippi; and I do not suppose that in an estuary marine influences prevail over the fluviatile, in order to foster the life of any of the creatures that have left their remains in these calcareous clays and sands; so that it may be said to be "partially of marine genesis." The same views here expressed by Dr. Dall were indicated by myself in another paper published by the Geological Survey of Alabama on the "Nita Crevasse" in 1889, in which I speak of the progress of later formations on and in the Mississippi Sound and its older extension as presenting a "marine-aspect" of the "Port-Hudson group" of Dr. Hilgard, and sufficiently different to be called the Biloxi Formation — a nomenclature I understood to have been approved by him among others. The method of genesis sketched in that paper for the Port Hudson was considered applicable to the older Post-Eocene formations of the same embayment.

I do not perceive, therefore, that Dr. Dall's "correction of my definition of these clays" was "required;" nor have I any to make of his, for similar views have been elaborated for the forthcoming Alabama Geological Report, which will be in effect a new edition of Bulletin 37 of the United States Geological Survey.

The only criticism here to which Dr. Dall might seem amenable is a tacit endorsement of his own brochure of January last upon these same Miocene formations, in which it may be said he has permitted conjecture upon general principles somewhat to outrun and forestall positive discovery. Hasty generalization is the bane of science. The Pascagoula Clays may be equivalent to his Chesapeake, but the testimony as yet can scarcely be said to be satisfactory. Whilst he has shown the younger Miocene of northern Florida, originally named by me the Waldo Formation, phases of which are seen at White Springs, in Hamilton County, and in the overlying clays at Aspalâga on the Apalachicola River, to be Chesapeake; this surely cannot be identical with the upper layers at Alum Bluff, much less with the lower.[2] As he himself has shown, the latter is an older Miocene, identical with that occurring on Chipola at Bailey's Bridge, and called by myself Chipola at a time when, from high water, I had not seen the *Ortholax beds* at Alum Bluff, and when I had not seen the perfect instance of contact and overlap presented at that place. At that time, I had previously discovered a Miocene in the vicinity of Defuniak Springs, on Shoal River, and on Alaqua River (and named it from the last), tracing it across Choctawhatchie, near Knox Hill, and across Washington County a little south of Vernon, and across Chipola at Abe Springs, eight miles south of Ten-Mile Bayou, the principal site of the older Miocene. With the help of Mr. Jüssen (both of us then working with Mr. Geo. H. Eldridge on the geological

[1] See Dr. J. W. Spencer's First Report of the Geological Survey of Georgia, p. 60; and short papers of my own, read severally at the meetings of the Geological Society of America, August, 1891, and August, 1892.

[2] There is no fossiliferous formation at Hawthorne, nor any at Ocheesee, as Dr. Dall seems to suppose.